

not authority the arbiter in cases of doubt, are the conclusions which the author inculcates throughout.

A century ago it was considered a fundamental principle that venesection was essential in most, if not all, serious illnesses; and, to such an extent was this carried, that 200 ounces of blood were sometimes drawn off during a week, and even half that amount in 24 hours. Next came a reaction, and the theory that fever patients required stimulation, rather than venesection, led to the administration of enormous quantities of alcohol, especially at the hands of Dr. Todd, who at times administered more than four gallons of brandy to young girls during an illness. Finally, to Dr. Gairdner himself is due much of the credit of the modern treatment; for in 1864 he showed that in fevers, especially typhus, the mortality is far less when the patients are supported with milk and not with alcohol. Quackery and humbug meet with but little mercy at the author's hands, and the hollowness of the pretensions of homœopathy is well brought out in an essay contributed thirty years ago, which is reprinted in this collection.

The volume should meet with a large circle of readers outside the medical profession, as it is eminently readable and touches upon many points in the past history of medicine as well as in modern practice, which are of interest to all.

Materials for a Flora of the Malayan Peninsula. Part I.

By Dr. George King, F.R.S., Calcutta. Pp. 50.
(Reprinted from the *Journal of the Asiatic Society of Bengal*, 1889, No. 4.)

SIR J. D. HOOKER'S "Flora of British India," of which five volumes out of seven are now printed, marks an era in tropical botany, inasmuch as it will probably contain descriptions, with their synonymy, of half the tropical plants of the Old World. It furnishes, therefore, a broad platform for his successors to build upon. It is not likely that within the bounds of India proper many new plants still remain to be described; but it is not so in the wonderfully rich flora of the Malay peninsula. During the last ten years large collections have been accumulated at Calcutta from this region, gathered mainly by Scortechini and other collectors who have been sent out by the authorities of the Calcutta Botanic Garden. In the present pamphlet, which is reprinted from the *Journal of the Asiatic Society of Bengal*, Dr. King, the Director of the Calcutta Garden, begins a synopsis of the plants which are indigenous to the British provinces of the Malay peninsula, including the islands of Singapore, Penang, and the Nicobar and Andaman groups.

In this present paper he deals with the orders Ranunculaceæ, Dilleniaceæ, Magnoliaceæ, Menispermaceæ, Nymphaeaceæ, Capparidææ, and Violaceæ, leaving over the intricate and largely represented order Anonaceæ for another time. In these seven orders there are 35 Malayan genera and 90 species, of which 32 are here described for the first time. Amongst the novelties are included a *Magnolia*, a *Manglietia*, 3 *Talaumas*, an *Illicium*, 4 species of *Capparis*, and no less than 11 new *Alsodeias*. Besides the species here described for the first time, there are several others, known previously in Java and China, which are new to British India. It will be seen that the work will add materially to our knowledge of Indian plants, and it is to be hoped that Dr. King, in the midst of his multifarious official duties, may be able to go on with it quickly and steadily. It is hardly worth while, we think, in a series of papers of this kind, to take up space and time by recapitulating in detail the characters of the orders and genera, as, from the nature of the case, it is essentially a supplement to Hooker's "Flora of British India," in which they are already fully worked out.

J. G. B.

LETTERS TO THE EDITOR.

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Panmixia.

SEEING that the whole structure of Prof. Weismann's theory is founded—both logically and historically—upon the doctrine of "panmixia," and seeing that in some important respects his statement of the doctrine appears to me demonstrably erroneous, I propose to supply a paper on the subject.

It will be remembered that the principal evidence on which Mr. Darwin relied to prove the inheritance of acquired characters was that which he derived from the apparently inherited effects of use and disuse—especially as regards the bones of our domesticated animals when compared with the corresponding bones of ancestral stocks in a state of nature. Now, in all his investigations regarding this matter, the increase or decrease of a part was estimated, not by directly comparing, say, the wing bones of a domesticated duck with the wing-bones of a wild duck, but by comparing the *ratio* between the wing and leg bones of a tame duck with the *ratio* between the wing and leg bones of a wild duck. Consequently, if there be any reason to doubt the supposition that a really inherited diminution of a part thus estimated is due to the inherited effects of diminished use, such a doubt will also require to extend to the evidence of a really inherited augmentation of a part being due to the inherited effects of augmented use. Now, there is the gravest possible doubt lying against the supposition that any really inherited decrease is due to the inherited effects of disuse. For it may be—and, at any rate to a large extent, must be—due to another principle which it is remarkably strange that Mr. Darwin should have overlooked. This is the principle of what Prof. Weismann has called panmixia. If any structure which was originally built up by natural selection on account of its use, ceases any longer to be of so much use, in whatever degree it so ceases to be of use, in that degree will the premium before set upon it by natural selection be withdrawn. And the consequence of this withdrawal of selection as regards that particular part will be to allow the part in a corresponding measure to degenerate through successive generations. Weismann calls this principle panmixia, because, by such withdrawal of natural selection from any particular part, promiscuous breeding ensues with regard to that part. And it is easy to see that this principle must be one of great importance in nature, inasmuch as it must necessarily come into operation in all cases where a structure or an instinct has ceased to be useful. It is likewise easy to see that its effects—viz. of inducing degeneration—must be precisely the same as those which were attributed by Mr. Darwin to the inherited effects of disuse; and, therefore, that most of the evidence on which he relied to prove the inherited effects both of use and of disuse is vitiated by the fact that the idea of panmixia never happened to occur to him. In this connection, however, it requires to be stated that the idea first of all occurred to myself, unfortunately just after the appearance of his last edition of the "Origin of Species." I then published in these columns a somewhat detailed exposition of the subject (see NATURE, vol. ix. pp. 361, 440, vol. x. p. 164). I called the principle the cessation of selection—which still seems to me a better, because a more descriptive, term than panmixia—and at first it appeared to me, as it now appears to Weismann, entirely to supersede the necessity of supposing that the effects of use and of disuse are ever inherited in any degree at all. Thus it obviously raised the whole question touching the admissibility of the Lamarckian principles in any case, or the question which is now being so much discussed concerning the possible inheritance of acquired as distinguished from congenital characters. But Mr. Darwin satisfied me that this larger question could not be raised. That is to say, although he fully accepted the principle of panmixia, and as fully acknowledged its obvious importance, he left no doubt in my mind that there was independent evidence for the transmission of acquired characters sufficient in amount to leave the general structure of his previous theory unaffected by what he nevertheless recognized as a necessarily additional factor in it. And forasmuch as no further facts bearing upon the subject have been forthcoming since that time, I see no reason to change the judgment that was then formed.

There is, however, one respect in which Prof. Weismann's statement of the principle of panmixia differs from that which was considered by Mr. Darwin; and it is this difference of statement—which amounts to an important difference of theory—that I now wish to discuss.

The difference in question is, that while Prof. Weismann believes the cessation of selection to be capable of inducing degeneration down to the almost complete disappearance of a rudimentary organ, I have argued that, *unless assisted by some other principle*, it can at most only reduce the degenerating organ to considerably above one-half its original size—or probably not through so much as one-quarter. The ground of this argument (which is given in detail in the NATURE articles before alluded to) is, that panmixia depends for its action upon fortuitous variations round an ever-diminishing average—the average thus diminishing because it is no longer *sustained* by natural selection. But although no longer sustained by *natural selection*, it does continue to be sustained by *heredity*; and therefore, as long as the force of heredity persists unimpaired, fortuitous variations alone—or variation which is no longer controlled by natural selection—cannot reduce the dwindling organ to so much as one-half of its original size; indeed, as above foreshadowed, the balance between the positive force of heredity and the negative effects of promiscuous variability will probably be arrived at considerably above the middle line thus indicated. Only if for any reason the force of heredity begins to fail, can the average round which the cessation of selection works become a progressively diminishing average. In other words, so long as the original force of heredity as regards the useless organ remains unimpaired, the mere withdrawal of selection cannot reduce the organ much below the level of efficiency above which it was previously *maintained* by the *presence* of selection. If we take this level to be 70 per cent. of the original size, cessation of selection will reduce the organ through the 30 per cent., and there leave it fluctuating about this average, unless for any reason the force of heredity begins to fail—in which case, of course, the average will progressively fall in proportion to the progressive weakening of this force.

Now, according to my views, the force of heredity under such circumstances is always bound to fail, and this for two reasons. In the first place, it must usually happen that when an organ becomes useless, natural selection as regards that organ will not only *cease*, but become *reversed*. For the organ is now absorbing nutriment, causing weight, occupying space, and so on, *uselessly*. Hence, even if it be not also a source of actual danger, "economy of growth" will determine a reversal of selection against an organ which is now not merely useless, but deleterious. And this degenerating influence of the reversal of selection will throughout be assisted by the cessation of selection, which will now be always acting round a continuously sinking average. Nevertheless, a point of balance will eventually be reached in this case, just as it was in the previous case where the cessation of selection was supposed to be working alone. For, where the reversal of selection has reduced the diminishing organ to so minute a size that its presence is no longer a source of detriment to the organism, the cessation of selection will carry the reduction a small degree further; and then the organ will remain as a "rudiment." And so it will remain permanently, unless there be some further reason why the still remaining force of heredity should be abolished. This further reason I found in the consideration that, however enduring we may suppose the force of heredity to be, it would be unreasonable to suppose that it is actually everlasting; and, therefore, that we may reasonably attribute the eventual disappearance of rudimentary organs to the eventual failure of heredity itself. In support of this view there is the fact that rudimentary organs, although very persistent, are not everlasting. That they should be very persistent is what we should expect, if the hold which heredity has upon them is great in proportion to the time during which they were originally useful, and so firmly stamped upon the organization by natural selection causing them to be strongly inherited in the first instance. Thus, for example, we might expect that it would be more difficult finally to eradicate the rudiment of a wing than the rudiment of a feather; and accordingly we find it a general rule that long-enduring rudiments are rudiments of organs distinctive of the higher taxonomic divisions—*i.e.* of organs which were longest in building up in the first place, and longest sustained in a state of working efficiency in the second place. Again, that rudimentary organs, although in such cases very

persistent, should not be everlasting, is also what we should expect, unless (like Weismann) we have some argumentative reason to sustain the doctrine that the force of heredity is inexhaustible, so that never in any case can it become enfeebled by a mere lapse of time—a doctrine the validity of which in the present connection I will consider later on.

Thus, upon the whole, my view of the facts of degeneration remains the same as it was when first published in these columns sixteen years ago, and may be summarized as follows.

The cessation of selection when working alone (as it probably does work in our domesticated animals, and during the first centuries of its working upon structures or colours which do not entail any danger to, or perceptible drain upon the nutritive resources of, the organism) cannot cause degeneration below, probably, some 20 to 30 per cent. But if from the first the cessation of selection has been assisted by the *reversal* of selection (on account of the degenerating structure having originally been of a size sufficient to entail a perceptible drain on the nutritive resources of the organism, having now become a source of danger, and so forth), the two principles acting together will continue to reduce the ever-diminishing structure down to the point at which its presence is no longer a perceptible disadvantage to the species. When that point is reached, the reversal of selection will terminate, and the cessation of selection will not then be able of itself to reduce the organ through more than at most a very few further percentages of its original size. But, after this point has been reached, the now total absence of selection, either for or against the organ, will sooner or later entail this further and most important consequence—*viz.* a failure of heredity as regards the organ. So long as the organ was of use, its efficiency was constantly *maintained* by the *presence* of selection—which is merely another way of saying that selection was constantly maintaining the force of heredity as regards that organ. But as soon as the organ ceased to be of use, selection ceased to maintain the force of heredity; and thus, sooner or later, that force began to waver or fade. Now it is this wavering or fading of the force of heredity, thus originally due to the cessation of selection, that in turn *co-operates* with the still continued cessation of selection (panmixia) in reducing the structure below the level where its reduction was left by the actual reversal of selection. So that from that level downwards the cessation of selection and the consequent failing of heredity act and react in their common work of causing obsolescence. In the case of newly acquired characters the force of heredity will be less than in that of more anciently acquired characters; and thus we can understand the long endurance of "vestiges" characteristic of the higher taxonomic divisions, as compared with those characteristic of the lower. But in all cases, if time enough be allowed, under the cessation of selection the force of heredity will eventually fall to zero, when the hitherto obsolescent structure will finally become obsolete.¹

Let us now turn to Weismann's view of degeneration. First of all, he has omitted to perceive that "panmixia" alone (if unassisted either by reversed selection or an inherent diminishing of the force of heredity) cannot reduce a functionless organ to the condition of a *rudiment*. Therefore he everywhere represents panmixia (or the mere *cessation* of selection) as of itself sufficient to cause degeneration, say from 100 to 5, instead of from 100 to 80 or 70, which, for the reasons above given, appeared (and still appears) to me about the most that this principle alone can accomplish, so long as the original force of heredity continues unimpaired. No doubt we have here what must be regarded as a mere oversight on the part of Prof. Weismann; but the oversight is rendered remarkable by the fact that he *does* invoke the aid of reversed selection *in order to explain the final disappearance of a rudiment*. Yet it is self-evident that the reversal of selection must be much more active during the initial than during the final stages of degeneration, seeing that, *ex hypothesi*, the greater the degree of reduction which has been attained the less must be the detriment arising from any useless expenditure of nutrition, &c.

And this leads me to a second oversight in Prof. Weismann's statement, which is of more importance than the first. For the

¹ It may not be needless to add that in the case of newly acquired and comparatively trivial characters, with regard to which reversal of selection is not likely to take place (*e.g.* slight differences of colour between allied species), cessation of selection is likely to be very soon assisted by a failure in the force of heredity; seeing that such newly acquired characters will not be so strongly inherited as are the more ancient characters distinctive of higher taxonomic groups.

place at which he does invoke the assistance of reversed selection is exactly the place at which reversed selection must necessarily have ceased to act. This place, as already explained, is where an obsolescent organ has become rudimentary, or, as above supposed, reduced to 5 per cent. of its original size; and the reason why he invokes the aid of reversed selection at this place is in order to save his doctrine of "the stability of germ-plasm." That the force of heredity should finally become exhausted if no longer maintained by the presence of selection, is what Darwin's theory of perishable gemmules would expect to be the case, while such a fact would be fatal to Weismann's theory of an imperishable germ-plasm. Therefore he seeks to explain the eventual failure of heredity (which is certainly a fact) by supposing that after the point at which the cessation of selection alone can no longer act (and which his first oversight has placed some 70 per cent. too low), the reversal of selection will begin to act directly against the force of heredity as regards the diminishing organ, until such direct action of reversed selection will have removed the organ altogether. Or, in his own words, "The complete disappearance of a rudimentary organ can only take place by the operation of natural selection; this principle will lead to its diminution, inasmuch as the disappearing structure takes the place and the nutriment of other useful and important organs." That is to say, the rudimentary organ finally disappears, not because the force of heredity is finally exhausted, but because natural selection has begun to utilize this force against the continuance of the organ—always picking out those congenital variations of the organ which are of smallest size, and thus, by its now reversed action, reversing the force of heredity as regards the organ.

Now, the oversight here is that the smaller the disappearing structure becomes, the less hold must "this principle" of reversed selection retain upon it. As above observed, during the earlier stages of reduction (or while co-operating with the cessation of selection) the reversal of selection will be at its maximum of efficiency; but, as the process of diminution continues, a point must eventually be reached at which the reversal of selection can no longer act. Take the original mass of a now obsolescent organ in relation to that of the entire organism of which it then formed a part to be represented by the ratio 1 : 100. For the sake of argument we may assume that the mass of the organism has throughout remained constant, and that by "mass" in both cases is meant capacity for absorbing nutriment, causing weight, occupying space, and so forth. Now, we may further assume that when the mass of the organ stood to that of its organism in the ratio of 1 : 100, natural selection was strongly reversed with respect to the organ. But when this ratio fell to 1 : 1000, the activity of such reversal must have become enormously diminished, even if it still continued to exercise any influence at all. For we must remember, on the one hand, that the reversal of selection can only act so long as the presence of a diminishing organ continues to be so injurious that variations in its size are matters of life and death in the struggle for existence; and, on the other hand, that natural selection in the case of the diminishing organ does not have reference to the presence and the absence of the organ, but only to such variations in its mass as any given generation may supply. Now, the process of reduction does not end even at 1 : 1000. It goes on to 1 : 10,000, and eventually 1 : ∞ . Consequently, however great our faith in natural selection may be, a point must eventually come for all of us at which we can no longer believe that the reduction of an obsolescent organ is due to this cause. And I cannot doubt that if Prof. Weismann had sufficiently considered the matter, he would not have committed himself to the statement that "the complete disappearance of a rudimentary organ can only take place by the operation of natural selection."

According to my view of the matter, the complete disappearance of a rudimentary organ can only take place by the cessation of natural selection, which permits the eventual exhaustion of heredity, when heredity is thus simply left to itself. During all the earlier stages of reduction, the cessation of positive selection was assisted in its work by the activity of negative or reversed selection; but when the rudiment became too small for such assistance any longer to be supplied, the rudiment persisted in that greatly reduced condition until the force of heredity with regard to it was eventually worn out. This appears to me, as it appeared to me in 1874, the only reasonable conclusion that can be drawn from the facts. And it is because this conclusion is fatal to Prof. Weismann's doctrine of the permanent "stability" of germ-plasm, while quite in accordance with all

theories which belong to the family of pangenesis, that I deem the facts of degeneration of great importance as tests between these rival interpretations of the facts of heredity. It is on this account that I have occupied so much space with the foregoing discussion; and I shall be glad to ascertain whether any of the followers of Prof. Weismann are able to controvert the views which I have thus re-published.

London, February 4.

GEORGE J. ROMANES.

P.S.—Since the above article was sent in, Prof. Weismann has published in these columns (February 6) his reply to a criticism by Prof. Vines (October 24, 1889). In this reply he appears to have considerably modified his views on the theory of degeneration; for while in his essays he says (as in the passage above quoted) that "the complete disappearance of a rudimentary organ can only take place by the operation of natural selection"—i.e. only by the reversal of selection,—in his reply to Prof. Vines he says, "I believe that I have proved that organs no longer in use become rudimentary, and must finally disappear, solely by 'panmixia'; not through the direct action of disuse, but because natural selection no longer sustains their standard structure"—i.e. solely by the cessation of selection. Obviously, there is here a flat contradiction. If Prof. Weismann now believes that a rudimentary organ "must finally disappear solely" through the withdrawal of selection, he has abandoned his previous belief that "the complete disappearance of a rudimentary organ can only take place by the operation of selection." And this change of belief on his part is a matter of the highest importance to his system of theories as a whole, since it betokens a surrender of his doctrine of the "stability" of germ-plasm—or of the virtually everlasting persistence of the force of heredity, and the consequent necessity for a reversal of this force itself (by natural selection placing its premium on minus instead of on plus variations) in order that a rudimentary organ should finally disappear. In other words, it now seems he no longer believes that the force of heredity in one direction (that of sustaining a rudimentary organ) can only be abolished by the active influence of natural selection determining this force in the opposite direction (that of removing a rudimentary organ). It seems he now believes that the force of heredity, if merely left to itself by the withdrawal of natural selection altogether, will sooner or later become exhausted through the mere lapse of time. This, of course, is in all respects my own theory of the matter as originally published in these columns; but I do not see how it is to be reconciled with Prof. Weismann's doctrine of so high a degree of stability on the part of germ-plasm, that we must look to the Protozoa and the Protophyta for the original source of congenital variations as now exhibited by the Metazoa and Metaphyta. Nevertheless, and so far as the philosophy of degeneration is concerned, I shall be very glad if (as it now appears) Prof. Weismann's more recent contemplation has brought his principle of panmixia into exact coincidence with that of my cessation of selection.—G. J. R.

Newton in Perspective.

THE interesting modern science termed by the Germans *Geometrie der Lage*, and by the French and other Latin peoples *géométrie de position*, may be traced in germ to that part of Newton's "Principia" which deals with the construction of curves of the second order, and to what has survived in tradition of Pascal's lost manuscript entitled "Traité complet des Coniques." The more recent developments of this important subject cast much new light upon Newton's propositions, many of which we are now enabled to solve by easier and more direct methods. A noteworthy example is here fully worked out, in order to show how problems which Newton solved by indirect and circuitous processes may be solved more simply by the aid of modern graphics.

PROBLEM.—Given the four tangents EA , AB , BC' , $C'D$ (Fig. 1), as well as a point of contact; to construct the conic.—First it will be necessary to give some faint idea of Newton's solution of this problem, without entering upon details which can be found in the Latin edition of the "Principia" edited by Sir William Thomson and Prof. H. Blackburn. Having expounded at great length a general theorem for the transformation of curves, Newton transforms the quadrilateral figure formed by the four tangents into a parallelogram. Then he joins the given point of contact y , transformed according to the same principle as the given four tangents, to the centre O of the parallelogram